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Constructs and Events in Verbal Behavior

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Skinner's (1957) analysis of verbal behavior has been the subject of much controversy in recent years. While criticism has historically come from outside the field of behavior analysis, there are now well-articulated arguments against Skinner's analysis of verbal behavior from within the field as well. Recently, advocates of Skinner's analysis have attempted to respond to the critiques, particularly to those regarding Skinner's definition of verbal behavior articulated by proponents of relational frame theory. Specifically, it has been suggested that talk about definitions equates to making the essentialist error. This paper provides an overview of these issues in the context of understanding the role of constructs in science more generally. It will be argued that definitions are central to scientific progress, and are not only relevant to a functional analysis, but a central prerequisite to the pursuit of such an analysis.

Key words: B. F. Skinner, interbehaviorism, relational frame theory, verbal behavior

Criticism surrounding Skinner's (1957) analysis of verbal behavior is now common, not only within the general fields of linguistics and psychology, but also within the behavior analytic community itself (see Gross & Fox, 2009). In this paper I will address criticisms regarding Skinner's definition of verbal behavior, and also review recent responses from advocates of Skinner's analysis of verbal behavior. In doing so, I will provide an overview of the role of constructs, and the common confusion between constructs and events in science. Finally, I will review the implications of this distinction for continued system building in behavior analysis and the area of verbal behavior specifically.

SKINNER'S ANALYSIS OF VERBAL BEHAVIOR

Skinner's (1957) analysis of verbal behavior was a seminal and prolific effort to understand language from a behavior analytic, functional perspective. Many behavior analysts consider it to be an important text; and it has no doubt influenced the field tremendously and continues to impact various research programs and assessment and treatment strategies (e.g., Sautter & LeBlanc, 2006; Schlinger, 2008; Sundberg & Partington, 1998). Furthermore, in recent times, behavior analysts interested in verbal behavior have elaborated upon Skinner's analysis. These elaborations have taken a range of forms and have been pursued for various purposes. While all have the general aim of improving behavior analytic approaches to language and cognition, some have been more critical of Skinner than others. Most notably, proponents of relational frame theory (RFT; S. C. Hayes, Barnes-Holmes, & Roche, 2001) have outlined several concerns with Skinner's analysis, and especially his definition of verbal behavior. These concerns are now well known and accepted within groups of behavior analysts who embrace RFT. (See Gross & Fox, 2009; S. C. Hayes & Barnes-Holmes, 2004; or S. C. Hayes et al., 2001 for a more thorough description of the concerns with Skinner's analysis of verbal behavior.) Central to this commentary is the specific criticism of Skinner's definition of verbal behavior. As such, I will provide a brief overview of this criticism and its implications below.

A primary concern with Skinner's (1957) definition of verbal behavior, as highlighted by RFT theorists, is that it is not functional in nature. Skinner's analysis of verbal behavior distinguished verbal behavior from nonverbal behavior by its indirect effects on the environment, by its effects on the behavior

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of the listener rather than the physical environment directly. An example provided by Skinner is that getting a glass of water yourself is considered nonverbal, whereas asking someone else to get you a glass of water is considered verbal, though both are reinforced by the water (p. 2). Specifically, per Skinner's definition, the latter behavior is considered verbal because reinforcement is mediated by a listener. Importantly, given this definition, ''[a]ny movement capable of affecting another organism may be verbal'' (Skinner, p. 14), not just vocal behavior. Moreover, the listener's behavior must have been explicitly conditioned to mediate reinforcement for the speaker's behavior for the speaker's behavior to be considered verbal.

Therefore, while behavior analysis typically aims to understand and classify behavior according to the environmental events participating in its occurrence, per Skinner's (1957) definition of verbal behavior, the classification of behavior as verbal depends upon a different behavior, that of the listener mediating reinforcement. An analysis of a rat pressing a lever exemplifies this issue. Is the rat's lever press to be considered verbal or nonverbal? Based upon Skinner's definition of verbal behavior, whether or not a rat pressing a lever is to be considered verbal or nonverbal has nothing to do with the lever press itself, but rather, depends upon whether or not the reinforcement for the lever press is mediated by a listener or not (i.e., was reinforcement mediated by an experimenter or automated?). Given this approach to defining verbal behavior, many responses might be considered verbal or nonverbal, and as a result, the study of verbal behavior itself might be put into question (see S. C. Hayes, et al., 2001, p. 14).

A second, related concern is the lack of a comprehensive research program stemming from Skinner's (1957) analysis. For instance, behavior analysts have found that much of the research derived from Skinner's analysis has focused on a small number of verbal operants (e.g., Sautter & LeBlanc, 2006) and populations with autism and developmental disabilities (e.g., Dixon, Small, & Rosales, 2007), leading some to question the ultimate impact of Skinner's analysis on the study and understanding of language (e.g., Dymond & Alonso-Álvarez, 2010). This issue is directly related to concerns with the definition of verbal behavior from the perspective of RFT theorists. Specifically, there is concern that Skinner's definition of verbal behavior may not have oriented behavior analysts to the unique features of verbal behavior, and therefore, led them right back to what they were already doing, thwarting a progressive and comprehensive research agenda on the important topics of language and cognition. The observation that Skinner's definition may not have oriented researchers to the unique features of verbal behavior is likely to be especially concerning to RFT theorists given their fundamental truth criterion centered on utility (e.g., S. C. Hayes, 1993). This could also be bothersome to some behavior analysts in general, as behavior analysts have pragmatic aims, generally speaking (see S. C. Hayes, Hayes, & Reese, 1988). In other words, definitions of verbal behavior should be useful to those attempting to understand and research the full range of verbal behavior.

The concerns with Skinner's (1957) definition of verbal behavior are well known and widely cited by those who do work related to RFT (e.g., Gross & Fox, 2009; S. C. Hayes & Barnes-Holmes, 2004). In fact, there may be somewhat of a theoretical divide between some behavior analysts and their approaches to language and cognition (e.g., see a recent discussion, Dymond & Alonso-Álvarez, 2010; Schlinger, 2008). Indeed, there are those who have extended Skinner's analysis (S. C. Hayes et al., 2001), those who seem to adhere closely to it (e.g., Palmer, 2004, 2008, 200; Schlinger, 2008), and those who integrate multiple perspectives (e.g., Barnes-Holmes, Barnes-Holmes, & Cullinan, 2000; Greer & Speckman, 2009; Rehfeldt & Root, 2005; Rosales & Rehfeldt, 2007). As RFT has gained popularity with both researchers and practitioners in behavior analysis (e.g., Dymond, May, Munnelly, & Hoon, 2010; Rehfeldt & Barnes-Holmes, 2009), advocates of Skinner's approach have begun to respond to the critiques of Skinner's definition made by RFT theorists. In the following section I will outline some of these responses.

RESPONSES TO THE CRITIQUES OF *VERBAL BEHAVIOR*

Generally speaking, advocates of Skinner's (1957) analysis acknowledge the con-

cerns with his definition of verbal behavior presented by RFT theorists (e.g., Normand, 2009; Palmer, 2004). Interestingly, that the definition is criticized for equating verbal behavior with simple acts of lever pressing is seen as the point of Skinner's analysis, not a criticism of it. For example, in responding to this criticism, Palmer (2004, p. 202) stated ''As for the breadth of Skinner's definition, the very point of his analysis is that verbal behavior is *not* different in kind from other behavior.'' Similarly, Normand (2009) has also defended Skinner's definition, indicating that the fact that it fails to orient us to anything other than what we were already doing is ''precisely the point, not the problem'' (p. 186). In other words, it has been argued that while Skinner's definition may do nothing more than suggest its similarity to other types of behavior, this is not to be viewed as a problem.[1] Apparently, the point being suggested is that a definition of verbal behavior is not needed to study verbal behavior. In fact, Normand (2009, p. 190) concluded his paper by stating, ''In essence, we could forego altogether any general definition of verbal behavior and proceed unimpeded into a functional analysis of language. Perhaps we should.''

Essentialism

Recently, the defense of Skinner's (1957) definition of verbal behavior has been taken a step further, with the suggestion that arguments over definitions represent a more fundamental error, essentialism (Normand, 2009, pp. 188–189). Specifically, it has been suggested that attempting to define verbal behavior is an indication of a fundamental error, rooted in the idea that there is something unique underlying verbal behavior that differentiates it from nonverbal behavior. Therefore, it is indirectly suggested that RFT theorists are committing the ''essentialistic error'' by focusing on the definition of verbal behavior. Moreover, it is argued that there is no natural difference between nonverbal and verbal behavior, and attempting to articulate one is therefore erroneous.

A paper by Palmer and Donohoe (1992) describes essentialism and its participation in psychology and behavior analysis. The authors describe essentialism as the idea that things are static and unchanging, and that categorical phenomena are viewed as representing enduring or ''real'' qualities of things (see p. 1345 for a discussion). Elaborating on this issue and criticizing a priori definitions, the authors suggest that, ''To the contrary, concepts with formal definitions *do not* map onto the kinds of the real world'' (p. 1347). In providing an alternative to essentialism the authors state ''… our attempt requires not the *imposing* of a definition but *discovering* one'' (p. 1348). Thus, as one can easily see, there is a general criticism of a priori definitions from this perspective. They see such definitions as problematic because they are based on a philosophical position that assumes that things have real and unchanging properties, which, when understood, must be classified.

Moreover, natural distinctions between events are said to be found in nature (Palmer & Donohoe, 1992, p. 1348), and thus, after finding such distinctions one may proceed to formulate a definition. As the authors state, ''Thus, the problem for the scientist is to determine the natural lines of fracture of the phenomena under study'' (p. 1348). In this paper, criticisms of cognitive psychology abound (e.g., the cognitive view of language), as well as of the behavior analytic distinctions between respondent and operant behavior and rule-governed and contingency-shaped behavior. Again, the idea proposed here is that each of these concepts represents the error of essentialism, the problematic idea that there exists a fundamental distinction among various constructs. The authors offer selectionism as an alternative to essentialism.

It is my view that selectionism is an approach (with selection being the process), and therefore cannot be offered as an *alternative* to a conceptualization of *what* it is that is being selected. Unfortunately, selectionism often seems to be embraced in the absence of an explication of exactly what it is that is being selected (e.g., Skinner, 1971); the result of which has been the

[1] Interestingly, Palmer (2008) seems to have changed his position at least somewhat in more recent times. Specifically, Palmer (2008, p. 297) has stated ''Behavior reinforced through the mediation of other people does indeed seem to be distinctive.''

overlooking of the subject-matter definition more generally. More specifically, contingencies of reinforcement do not define *what* it is that is being reinforced; operant contingencies do not define the subject-matter. While there are a number of important implications of the overlooking of the general definition of the subject-matter (e.g., reductionism and dualism), these issues are far beyond the scope of the current paper (but see L. J. Hayes & Fryling, 2009a, 2009b for an elaboration). Of relevance to the current discussion, the perspective that selectionism is an alternative to definitional constructs represents an error that results in neglecting the development and appreciation of constructs that can be used in the investigation of verbal behavior.

It is my position that the criticism of definitions described above represents confusion between constructs and events in behavior analysis. Before elaborating on this confusion in the context of the above-mentioned dialogue on verbal behavior I must first acknowledge the foundation of my perspective. My position is largely influenced by the works of J. R. Kantor and his philosophy of interbehaviorism (e.g., 1953), scientific system of interbehavioral psychology (1958), and related works (e.g., L. J. Hayes, 1992, 1993, 1994, 1997; Parrott, 1984). Interbehaviorists make a distinction between constructs and events and argue that these are often confused (e.g., Fryling & L. J. Hayes, 2009; Kantor, 1957; Smith, 2007) and that this confusion impacts the scientific arena. In the following section, I will provide an overview of constructs, paying special attention to issues pertinent to the conceptualization of verbal behavior.

CONSTRUCTS AND EVENTS

From the perspective of interbehaviorism, definitions are neither a priori *nor* after the fact. Scientific definitions are always based upon larger philosophical assumptions, which are always based on contacts with events (Kantor, 1957, 1969; Kantor & Smith, 1975). The philosophy of science is considered a scientific enterprise itself (Kantor, 1953, 1969), and thus, definitional acts are considered to be concrete scientific endeavors. Importantly, this sort of system building activity is not pursued with the aim of understanding something fundamental or ''essential'' about the event being defined. Rather, definitions serve important scientific purposes, namely to improve our orientation to and understanding of the subject-matter being studied. Thus, constructs are in general service of scientific work, supporting the central aims of the sciences (Kantor, 1953). Importantly, the more commonly noted behavior analytic goals of prediction and control (Skinner, 1953) are considered goals of particular subsystems of science, namely investigation and application. In other words, prediction and control cannot be pursued in the absence of an orientation to what it is that might be predicted and influenced. I will provide some context for this perspective below.

Interbehaviorism views the natural world as one integrated whole. Individual sciences attempt to understand parts of this whole, and do so by articulating a unique aspect of it, which is identified as their subject-matter (see L. J. Hayes & Fryling, 2009b). The fact that the subject-matters are derived from the same natural world results in their being viewed as *inter*dependent; indeed, no real distinction between any of the sciences is to be found in nature. Moreover, each of the sciences is therefore equally important, and irreducible to any of the others. As such, the selection of a subject-matter is arbitrary, although, once selected, the boundary between different domains is to be closely honored. The alternative to this is overlooking boundary conditions, whereby nothing unique may be added to the understanding of the matrix of natural happenings. In other words, if no unique subject-matter is identified and examined, the likelihood of something unique being added to scientific knowledge is diminished, and redundancy is made likely. This is a common occurrence in the sciences, primarily because such little attention is given to the articulation of the subject-matter in the first place. For example, behavior analysts often debate the extent to which behavior is a part of biology or sociology, whereby the unique contributions of each of the participating sciences might be overlooked or dismissed, and redundancy and disciplinary confusion made more common (L. J. Hayes & Fryling, 2009b). These

issues have grave consequences not only for disciplinary productivity, but they also prevent the possibility of productive interdisciplinary relationships. Put plainly, if one science is not adequately oriented to its subject-matter, how can that science add something to an interdisciplinary effort? (See L. J. Hayes & Fryling, 2009b for an elaborate discussion of these issues.)

To be sure, interbehaviorism assumes that the subject-matters of all sciences are always participating in each and every event. However, as no one science could ever understand the totality that is every event, unique subject-matters must be constructed to understand unique parts of this event field. To be clear, the subject-matters of individual sciences are constructed arbitrarily and therefore are not based upon essentialistic premises. Specific attention and adherence to these arbitrary definitions promotes internal validity (consistency), productivity, and interdisciplinary cooperation. In other words, *attention to definitions is fundamental*. Indeed, definitions will never ''map on'' to the natural world, they are constructions of it, and they will always fall short of the whole. In fact, one can never appreciate the whole and continue on as a scientist (see L. J. Hayes, 1993, 1997).

Constructs are also developed within disciplines, to orient workers to particular aspects of the subject-matter and to aid in investigation. For example, from the interbehavioral perspective, constructs such as operant and respondent conditioning are not viewed as independently operating processes, as nothing exists independently from anything (i.e., the world is an interrelated field). What the terms *respondent* and *operant conditioning* describe are particular patterns of behavior-environment relations, but the observation of one pattern in no way precludes the participation of another. In other words, the constructs ''operant'' and ''respondent'' in no way represent a fundamental, enduring quality of certain types of behavior; they are *descriptions* of patterns of behavior-environment interactions. Moreover, the distinction between operant and respondent behavior is not one that exists in nature, these terms are investigative constructions, nothing more. Thus, these conditioning processes aren't actual ''things,'' and therefore ''natural lines of fracture'' between them could never be found. Distinguishing between these processes is perfectly fine when the processes are viewed as investigative constructs. As such, conversation about operant-respondent interactions seems obvious; of course these constructs fail to be independent; the error was ever thinking they were in the first place. *This is not a problem with the constructs per se, but with confusing the constructs with the events of interest.* The same could be said for the distinction between rule-governed and contingency-shaped behavior; of course there aren't fundamental, enduring distinctions between these two types of happenings. Constructs are descriptions of part of the natural world. They never ''map on'' to it directly, they will always fall short of the whole. This is perfectly fine in behavior analysis, as it is within the sciences more generally, so long as the firm distinction between constructs and events is appreciated.

Defining Verbal Behavior

The consideration of constructs and events may aid in the continued debate and discussion surrounding the definition of verbal behavior. As mentioned above, the attention to Skinner's (1957) definition of verbal behavior has been considered to be essentialistic, and therefore problematic, or at least not helpful. The fact that we are unable to provide unambiguous and firm definitions of any type of behavior is proposed, and the example of aggression is given (Normand, 2009). Overlooked in this conversation is that definitions are *required* for any sort of inquiry. Indeed, the example of aggression requires a detailed operational definition (see, for example, the *Journal of Applied Behavior Analysis*). Such definitions are not essentialistic, they are operational, arbitrary, and *necessary* scientific constructions. There would be nothing to say about anything without them. Constructs are developed and used for particular purposes; they offer some advantage over existing constructs. This is consistent with the behavior analytic tradition more generally, as behavioral constructs were developed and employed because they offered some advantage over more traditional constructs. Certainly, no functional analysis

of any behavior could ever be pursued without a definition.

Consistent with the above, it seems that attention to definitions in behavior analysis, including that given to the definition of verbal behavior by workers in RFT or by Skinner himself, is unlikely to involve essentialism. Surely, workers in RFT do not believe verbal behavior has something ''essential'' about it, some underlying essence that is to be captured. All behavior analysts already know that verbal behavior is behavior. The question is what are its unique features? Just as the analysis of aggression requires an explication of its unique features relative to other behaviors, the analysis of verbal behavior warrants special treatment as well. All behaviors that are subjected to study require an orientation to what that behavior is and is not; indeed, no study could ever be conducted on ''behavior'' in a general sense; definitional constructs are critical.

MOVING FORWARD

If verbal behavior is not viewed as a unique *type of behavior* that warrants definition and special study, its special treatment by Skinner (1957) should also be criticized, not just its treatment by certain movements within behavior analysis. While Skinner's text does highlight the fact that verbal behavior is behavior, and not something cognitive, what else does it accomplish? Really, what is unique about verbal behavior? Are some of Skinner's adherents saying ''nothing''?

It is my perspective that while verbal behavior is just behavior, it is a unique type of behavior, with features that warrant special study. Put differently, while all behavior may be reduced to its bare similarities, the organization of psychological events of the verbal type seems to involve some special qualities. Specifically, verbal behavior seems to be substitutional, involving combinations of substitutional functions actualized in unique, multifactored fields (Kantor, 1924, 1958; Parrott, 1984). For example, a native Canadian living in Los Angeles might say ''I miss Canada,'' which is a response function that involves interacting with substitute stimulus functions (the individual is not actually in Canada, but interacting with it substitutionally via some present stimulus object). Moreover, the individual may be especially likely to interact with Canada substitutionally and engage in the response ''I miss the Canada'' on a holiday break, after a recent argument with a friend, after hearing a story about someone else missing their hometown, or while going for a walk on the ocean beach. (See Fryling & L. J. Hayes, 2010 for an analysis of reminiscing based on this perspective.) As highlighted by the above example, verbal behavior is distinct from other behavior in that the topography of the behavior has nothing to do with any physical features of the environment. In other words, we can interact, verbally, in any way with respect to any situation (e.g., you can think or say anything anywhere).[2] It seems unlikely that a lever press, or many of Skinner's (1957) verbal operants, will ever capture this quality of verbal behavior in the absence of further analysis and elaboration. *Definitions of verbal behavior should orient behavior analysts toward these issues.*

In this respect, RFT is to be commended, and not criticized, for attending to the unique qualities of verbal behavior. RFT deliberately attempts to deal with traditionally ''cognitive'' phenomena, and has informed a widely effective and broadly applicable psychotherapy approach, acceptance and commitment therapy (S. C. Hayes, Strosahl, & Wilson, 2011; Törneke, 2010). Indeed, the RFT constructs of mutual entailment, combinatorial entailment, and transformation of stimulus functions (S. C. Hayes et al., 2001, pp. 29–33) seem to bring us closer to the above mentioned aspects of verbal behavior. Similarly, the behaviors involved in ''relational framing'' and the stimulus properties involved in relational stimulus control, must be acknowledged as efforts to construct an alternative definition. My point is that attention to constructs has been useful toward the understanding and development of a science of behavior that targets the full range of language events. Nothing ''essentialistic,''

[2] In Kantor's terms, this involves both stimulus and response substitution (i.e., implicit responding; e.g., Kantor, 1924, 1926; also see Fryling, 2012).

dualistic, or ''mediational'' is ever suggested in this analysis.

At the same time, I do not think anyone should dismiss any aspect of our history, such as Skinner's (1957) analysis of verbal behavior. Clearly, it has been useful in some areas, and will likely continue to be so (e.g., Dixon et al., 2007; Sautter & LeBlanc, 2006). If Skinner's analysis of verbal behavior is flawed, RFT certainly brought attention to important issues, issues that might help behavior analysis move forward as a field. This does not require the field to abandon Skinner's analysis, but instead to consider it as *one possible* analysis, nothing more. Moreover, I encourage those interested in verbal behavior to consider whether the progress made by RFT in a relatively short amount of time may be related to the willingness of its proponents to think critically about the conceptual foundation of verbal behavior, and to formulate a specific and comprehensive research agenda. Continued dialogue regarding these issues must be constructive and oriented toward further system building, toward the progress of behavior analysis as a *field*.

To conclude, I am not suggesting we throw out Skinner's (1957) *Verbal Behavior.* Furthermore, I am not necessarily suggesting we all become advocates of RFT (see Fryling, 2012). I am advocating for behavior analysis as a discipline, a coordinated scientific enterprise. Researchers in verbal behavior should seriously consider their goals in this area, as well as how these goals can be best accomplished. It is possible that some individuals have the goal of making sure that Skinner's work is carried on and viewed as the primary treatment of language by behavior analysts. If so, there is work to be done. As stated by Dixon et al. (2007), ''Until researchers construct experiments that more closely resemble the complexities of human conversation, there will be doubters of Skinner's approach within and beyond the behavior-analytic community'' (p. 205). Indeed, doubters may grow in number as modern alternatives to Skinner's analysis thrive.

Finally, in behavior analysis we need to be careful not to confuse constructs with events, and begin to appreciate the role of *scientific* philosophy more fully (Kantor, 1969). If behavior analysis were to pursue scientific system building along the lines suggested in this paper a more productive dialogue might ensue, whereby the likelihood of coordinative, discovery-oriented research may increase. Sharper distinctions between constructs and events, various types of constructs, and more might be made. The framework for such coordinative work was proposed by J. R. Kantor (1958), and has much to offer workers in behavior analysis. I hope my comments have served to increase the probability of constructive system building in behavior analysis.

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